

Panizzi agreed to these terms, and offered "to wait on the Committee, as soon as convenient to them, to settle the manner in which they wish the work to be executed."

Now the whole gist of this quarrel consists in this, that the Library Committee naturally wished to control Mr. Panizzi in his mode of executing the work, while he refused to be controlled or interfered with in any manner. He even regarded as personal enemies all those who attempted so to interfere. He fancied that every one who differed from him was actuated by a sense of personal dislike. When he refused Dr. Roget's request to revise the sheets of the Catalogue, he says (p. 6): "I had no idea when I so candidly expressed my opinion that I was making a powerful and unrelenting enemy in one of the most influential officers of the Royal Society." At p. 51 he says: "so gratuitous an insult would never have been allowed had not Mr. Bailey filled the chair at that meeting." And again (p. 5), "My statements will be received with derision by those who know that they may be unjust with impunity." At p. 18 he charges the Committee with "indelicate conduct," at p. 22 with "absurdity," at p. 25 such things were done "purposely to annoy me;" and again, "No suggestion of mine would ever be attended to by the Council." At p. 26 his work was regarded with "a malignant eye;" at p. 28 "The annoyance was incessant," "injurious and unjust;" at p. 33, "treating me as if I were their servant," "unwarrantable liberty;" p. 38, "unjustly interfered with;" p. 41, "insulted with an order of submitting my work to revision. . . I shall never consent for any one, be he who he may, to make any alterations in it." And when, on June 24, 1836, he was requested to attend the Library Committee on the following Monday at 4 p.m., he declined on the ground that "when I attended before I was not so well satisfied with my position as to wish to be in it again." At p. 54, when clamouring for payment of an unascertained balance which he claimed, he charges the Council with not meaning "to pay it unless they be compelled to it. . . . Possibly there is some legal means of obtaining redress; but in a country like this justice is not a luxury for a poor man to indulge in; and the Council, having at their disposal the funds of the Royal Society, can amuse themselves without personal trouble or loss with a law-suit which I have not the means of sustaining." Will it be believed, in the face of such language as this that Panizzi had already been paid the sum of 450*l.*, and his whole remuneration was not to exceed 500*l.*

In his second pamphlet (p. 18), after charging the Council with not meaning to act fairly, he hurls at it his "unmixed disgust and contempt." But I cannot help thinking that these vigorous epithets would have been more appropriate had they travelled the other way.

When requested to return the printer's revises, and he refused on the ground that they were his own property, together with the key of a drawer in one of the Royal Society's rooms, and he also refused, what wonder that, after so long a contest with this cantankerous man, the Council should have resolved on July 14, 1837, "that Mr. Panizzi be no longer employed in the formation of the Catalogue."

The reader may well exclaim by this time, What *is* all this hubbub about? Simply this: Mr. Panizzi insisted on adding to some of the items of the Catalogue original comments of his own, to which the Library Committee justly objected as committing the Society to opinions of doubtful value. Panizzi attached the greatest importance to these notes and comments. "The Committee, far from objecting to them, ought to have been thankful that I had taken the trouble of introducing them" (p. 31); and he proceeds to quote specimens illustrative of this part of his work. For example, he says: "To the 'Mémoires' of Charnières on the observations of the longitude, I added this note: 'All the author's additions and corrections carefully put in by J. B.' This note is on the title-page of this copy, and the volume is interspersed with alterations in manuscript. I suppose J. B. to mean James Bradley." Later on in the same page he adds: "The author's additions, if put in by Bradley, are, of course, of much more value than if written by any other J. B."

Now the book in question is only a single *Mémoire* of De Charnières, not a collection of "*Mémoires*," as described by Panizzi. Moreover, there are five reasons why the additions and corrections could not have been written in by Dr. Bradley.

1. He died five years before the memoir by Dr. Charnières was published. This may well excuse the other four reasons, but they are curious as illustrating the carelessness of a man who was convinced of his own infallibility.

2. The writing of the anonymous J. B. is small and neat: that

of Bradley large and awkward. The Royal Society had in its possession manuscripts of Bradley and his signature, which could be seen by merely asking the assistant-secretary for them, and yet Panizzi did not submit the writing of J. B. to this simple test.

3. Bradley was not in the habit of writing in his books.

4. The so-called "additions and corrections" are simply the corrigenda collected into eight pages at the end of the book, and transferred in MSS. to the text, a fidgety piece of work, not likely to be undertaken by so busy a man as Bradley.

5. At the end of the book J. B. drops his incognito and appears as *J. Bevis*, a fact overlooked by Panizzi.

Other similar examples might be given, and indeed were submitted to the Fellows of the Royal Society at the time, in order to justify the resolution of the Library Committee "that all comments or notes expressing matters of opinion on the articles in the catalogue be omitted"; but the statement of them would occupy too much space, dealing as they do with details which unless given in full would not be understood.

Mr. Panizzi was undoubtedly a vigorous clever man; but in the matter of books, he, unfortunately for his own reputation, aspired to universal knowledge which belongs to no one. The gold of a universalist is apt to shrink down into dross when tested in the crucible of a specialist. Having occasion to consult a book by Gay-Lussac, and not finding it in the Catalogue of the British Museum Library, the attendant requested me to write the name and title on a slip and show it to Mr. Panizzi. No sooner had he glanced at the slip than he exclaimed "Ah! you have made a mistake: it is *Guy-Lussac*!" This readiness on all occasions to say something apparently to the purpose, may impress subordinates with a sense of power on the part of their chief, but to tell a chemist that *Gay-Lussac* is *Guy-Lussac* would be much the same as telling him that potash and soda are identical compounds.

C. TOMLINSON

Highgate, N., August 2

The Oldest Fossil Insects

In a paper on "The Devonian Insects of New Brunswick" (*Bull. Mus. Compar. Zoology*, 1881, vol. viii. No. 14) I have drawn attention to the fact that a fern on the same slab with *Platophemera* was determined in 1868 by Prof. Geinitz as *Pecopteris plumosa*, and therefore the slab considered by him as belonging to the Carboniferous. I believed that here an important gap was still to be filled, namely, the reliable determination of the fern, which is not mentioned in Mr. S. H. Scudder's monograph, nor in Principal Dawson's note on the geological relation of those insects, which closes Mr. Scudder's paper.

A paper by Mr. Dawson (*Canad. Naturalist*, 1881, vol. x. No. 2) is intended to fill this gap. The fern is after the study of the original specimen determined as *Pecopteris serrulata*, and said to be a common species in those beds. If I am not entirely mistaken it will be difficult to agree with Mr. Dawson's opinion (*l.c.* p. 2) "that doubts and suspicions thus cast on work carefully and exhaustively done should not seriously affect the minds of naturalists," as it happens that in his work of 1880 this common species is not quoted at all among the plants found in those beds, except in a note (p. 41) stating that in the beds 6 to 8 three or four other species occur, among them *probably P. serrulata*. Mr. Dawson quotes for the species the figures 207 to 209 in his Report of 1870, but I confess to be unable to recognise the *Platophemera* fern in those figures.

Prof. O. Heer has kindly drawn my attention to his "Flora Fossilis Arctica of Bear Island, Spitzbergen, 1871." He has given (pp. 14, 15) a detailed review of the fossil plants from St. John's, New Brunswick, and, as he still believes, has proven that those layers do not belong to the Devonian but to the Ursa stage of the Lower Carboniferous. This important and elaborate statement is disposed of by Mr. Dawson, as far as I know, only in his report, 1873, p. 8, in the following words:—"The so-called Ursa stage of Heer includes this (Lower Carboniferous), but he has united it with Devonian beds, so that the name cannot be used except for the local development of these beds at Bear Island."

It is true that Mr. Dawson, in the supplement to the third edition of the "Acadian Geology," 1878, p. 72, has tried to explain the different opinion of Prof. Heer by the earlier introduction of the Palæozoic flora in American formations. But this fact, known by every one, and of course by Prof. Heer, is not considered by him to be a sufficient objection to the statements given in the "Flora of Bear Island."

The paper of Prof. Heer states carefully and exhaustively the

facts which induced him to consider those layers at St. John's as belonging to the Lower Carboniferous. Therefore naturalists will scarcely agree that such a statement, made by a prominent and acknowledged authority, can be cancelled by a simple negation not supported by facts. Till this is done in a reliable manner, those oldest insects will have to be considered as belonging to the Lower Carboniferous.

Cambridge, Mass., July 25

H. A. HAGEN

The True Coefficient of Mortality

THE very interesting and suggestive lecture of Alexander Buchan on "The Weather and Health of London" (*NATURE*, vol. xxiv. p. 143 *et seq.*) reminds me of the propriety of calling the attention of writers on "vital statistics" to a point in relation to the true method of discussing the mortuary data. The specific point to which attention is drawn is the necessity of estimating the relative tendency to special diseases by comparing the number of deaths from the given cause with the number of persons living at the ages embraced in the record; instead of making the comparison (as is usually done) with the total deaths from all causes, or with the total number living at all ages.

In like manner, in discussing the influence of age on the mortality from any given disease, it is very common to prepare tables of the number of deaths at each age, and in some instances these numbers have been assumed to represent the relative tendency to the disease at different ages. It is scarcely necessary to say that this is a very serious error, for it must be borne in mind that the number of persons living at different ages is very unequal. Indeed it is self-evident that the true coefficient of mortality for any given disease at any given age is expressed by the ratio of the number of deaths from the specified disease at the given age to the number of persons living at the same age: or, as it may be otherwise indicated, the number of deaths from the given disease at the given age per 1000 persons living at the same age.

In illustrating this point I shall select cancer, because, in relation to the influence of age, it furnishes an extreme case, and thus affords a glaring instance of the fallacy of taking any basis of comparison other than the number of persons living at each age. The mortuary records of the Department of Seine in France, during the eleven years, from 1830 to 1840 inclusive, furnish a total of 9118 deaths from cancer, 2163 males and 6955 females. The following table relating to the mean annual mortality from this disease among females will illustrate this point:—

Age. Years.	Number of females living.	Mean annual deaths from cancer among females.	Annual deaths from cancer in 1000 females living at all ages.	Annual deaths from cancer in 1000 females living at each age.
(1)	(2)	(3)	(4)	(5)
0 to 10	139,840	1'273	—	0'00910
10 „ 20	115,269	1'182	—	0'01026
20 „ 30	104,342	15'364	0'04196	0'14725
30 „ 40	73,203	74'727	0'20409	1'02081
40 „ 50	54,124	148'727	0'40619	2'74788
50 „ 60	36,800	147'273	0'40221	4'00198
60 „ 70	25,703	133'545	0'36472	5'19564
70 „ 80	12,852	83'364	0'22767	6'48659
80 „ 90	3680	24'818	0'06778	6'74408
90 „ 100	340	2'000	0'00546	5'88769
All ages.	566,153	632'273	—	1'11679

The foregoing table demonstrates the inaccuracy of the popular impression that the tendency to cancer attains its maximum between the ages of 35 and 50 years. The numbers in columns (3) and (4) might seem to support such an opinion; but, as we have seen, those in column (5) are evidently the true indices of the tendency to this disease at different ages; and it will be observed that the mortality goes on steadily augmenting with each succeeding decade of age up to 90 years. The fact likely to be most strongly impressed on the reader by the numbers in column (5) is the remarkable regularity of increase of the co-

efficient of mortality for cancer with advancing life among females after the age of 25 or 30 years. Between the ages of 25 and 75 the mortality increases nearly in arithmetical progression as the age advances in arithmetical progression, the average increment being about 1'30 per 1000 living at each age for each decade. Assuming this to be the law of mortality from cancer among females, it admits of very simple mathematical expression. Thus, let

A = the age at which liability to cancer begins.

A' = any age greater than A.

C = constant coefficient, variable according to country, state of civilisation, &c.

Then we have—Annual mortality per 1000 living at age A' = C (A' - A).

In our table representing the mortality from cancer in the department of the Seine from 1830 to 1840 inclusive, the value of A may be taken = 25, and C = 0'13; hence we have—Annual mortality per 1000 living at age A' = 0'13 (A' - 25). Thus by the formula the mortality at 55 = 3'90, and column (5) gives 4'00 between 50 and 60; at 75, formula = 6'50; table = 6'49 between 70 and 80.

The mortality from cancer seems to be vastly smaller in England than it is in France, so that a less value must be given to the constant C. The foregoing formula represents the law of increasing mortality with advancing life in the simplest form, as a function of the age. This extreme simplicity is probably unique in the case of cancer, and seems to indicate that age is so far the controlling element in the development of this disease as to overpower all other causes. In the case of other diseases we cannot expect to escape the necessity of employing those exponential functions in investigating their laws of mortality, which are essential when a multiplicity of causes are in operation.

Many years ago the attention of the medical profession in this country was called to the fact that the available mortuary data were not discussed in a manner which revealed the true value of the facts contained in the numbers.¹ But there is reason to believe that Prof. Francis A. Walker, the intelligent superintendent of the census of the United States for 1880, will not overlook this point when he comes to the discussion of the mortuary statistics which have been collected.

Berkeley, California, July 7

JOHN LE CONTE

[Mr. Le Conte does not appear to have apprehended the point discussed in the lecture on "The Weather and Health of London"—that point in no part of the inquiry being the tendency to the disease at different ages, but the manner of the distribution of deaths in the case of each disease through the weeks of the year, with the view of arriving at some knowledge of the influence of season in determining that distribution. Only in one case, viz., in discussing the rates of the mortality from diarrhoea in several large towns, was a reference to population required, and in that case the curves were drawn, showing the weekly rate of mortality per 1000 of the population of the respective towns.—ALEXANDER BUCHAN.]

Bisected Humble Bees

I TOO have frequently observed humble bees lying dead or stupefied under lime-trees, sun-flowers, and some other plants, and once I saw a Staphylinus, commonly known as Black Cock-tail, or Devil's Coach-horse, nip a humble bee in two, and on passing that way later I found that it had cleared out the honey-bag and left the two halves of the bee on the path, as described by your correspondent. I have known boys catch humble bees and eat the honey in them; and probably many other animals have learned how to get at the sweet drop.

Trinity College, Cambridge

THOS. MCK. HUGHES

AT your request for information on the above I beg to say that I have observed both the flycatchers alluded to by your correspondent, and also the little blue tit (*Parus cerulea*) attack the humble bees in the manner described, to extract the honey-bag. This attacking the bees is not, so far as my experience goes, a general characteristic of these birds, and what should lead them to it occasionally I cannot ascertain.

Exeter, August 15

EDWARD PARFITT

¹ Vide papers by the writer, entitled "Statistical Researches on Cancer," *Southern Med. and Surg. Journ.*, new series, vol. ii. pp. 257-293, May 1846. Also "Vital Statistics," illustrated by the "Laws of Mortality from Cancer," *Western Lancet*, vol. i. pp. 176-190, March, 1872 (San Francisco).